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Rexford F. Daubenmire (1910-1995)

"Dauby" was the usual appellation applied by graduate students to Dr. Daubenmire, Professor at Washington State University, Pullman, Washington, during the years 1950-1953 while I was working under the aegis of the late Prof. Marion Ownbey (1910-1974) in the area of plant systematics.

I first read about Dauby's death in the obit section of the *New York Times* (8 September 1995). This was a short but well-written account of his professional life and contributions to ecology. Unfortunately it conveyed very little about the man himself. Indeed, most scientists are largely remembered by brief obits prepared by their professional colleagues in which their lives are summed up as lines culled from their latest CV. Subsequent biographers have to invent their other attributes, especially for scientists who are reluctant to write personal letters or expose their psyches.

Perhaps, for many workers, that is as it should be. But I feel otherwise. Indeed, the only previous obits to have been penned by me (Turner 1972, 1975) were both highly personal, although both were solicited. In these I wished to portray the inner essence of the person, his weaknesses and strengths, beauties, foibles, whatever. Whether or not I succeeded in these endeavors is not so important as the attempt, for these will surely provide future biographers with at least some material by which to humanize their subjects. To me, at least, an individual's work cannot be understood solely by publications and their contents.

The present obit is obviously unsolicited. It is written simply because I thought Dauby was a fine researcher, a commendable undergraduate teacher, and a remarkable professional. Certainly, any deep appreciation I have of the field of ecology comes from my enrollment in all of the courses he taught in botany at W.S.U. during the time of my attendance at that institution. These included autecology, synecology, field ecology, and plant geography; I also served as his T.A. in undergraduate courses in general botany, sitting in on all of his freshman lectures on that subject.

Dauby was, for the most part, a calm, even-tempered, rather handsome man. He wore a full mustache above a seemingly perpetual Gioconda-like smile (unusual for most competitive males of my acquaintance, at the time or since). Even when exceedingly irritated he retained that sphinxious grin: along with his expressive eyes, and thin lips, he exuded a detached serenity that belied his inner turmoils.

At the time I knew him, during the prime of his professional career, aged 40-43, Dauby was lean and well-proportioned, about 5 feet ten or so and perhaps 150 pounds. He wore an academic costume to all of his formal lectures: well-creased pants, a professorial tweed coat with leather covered elbows, bowtie, and freshly polished shoes. I remember this well, for the late Art Cronquist (1919-1992), his colleague at the time, for whom I was also a T.A., dressed in just the opposite fashion, usually a slip-over, much-abused sweater, baggy pants and coat, that looked slept in, occasionally an off-angled mussed tie, and large military-type shoes in various stages of repair. In short, Dauby believed in appearances; Art did not. Like their attires, they were antagonists, but most of the antagonism drifted downward from Dauby. I can still recall a brief statement or two made to himself by Dauby upon hearing the approach of Art along the lower floor of the botany building as Dauby ascended the stairs leading to the second floor, myself along his side. Cronquist, with his six foot eight inch Swedish frame, would usually enter the building with a large booming voice singing whatever song entered his mind, operetta or ballad. On this particular day it was "Oh, she jumped in bed and covered up her head and said I couldn't find her. . .." and carried on through the whole verse (which I myself sang upon occasion, having learned it as a teenager in Texas). Dauby paused for a second, looking at me with grimaced eyes and no smile, saying "That man! God, that man!" Then he trudged on up to the second floor with a perplexed expression.

In Dauby's formal undergraduate lectures he spoke at a slow clip, very precisely, everything biological presented as black or white, with little, if any, gray areas. He drew precise figures on the chalk board and labeled their parts with easily read names. Excellent teacher, answering questions from the floor briefly but adequately.

In upper undergraduate and graduate level courses he was less effective. For example, in autecology, having written the text himself, Dauby did not feel it necessary to lecture on the subject, rather he would meet his classes so as to answer questions about any ambiguities in the text chapters, which we were all expected to have pored over prior to attendance. Most of these classes lasted 10-15 minutes, though sometimes they were prolonged by an overly querulous student. This permitted him to shorten his teaching load and retire to his office (door nearly always closed) so that he might get on with his research or textbook writings.

Dauby took a different tack for his course in synecology (lectures from which he was hoping to develop a text on the subject, and did). He often became rather enlivened by his own spontaneous insights into the field of community ecology, holding forth on succession, its history, comparing community classification to systematic classification, but always with the admonition to accept such comparisons as "analogous to," not "the same as," *etc*. At such times he could be brilliant, but, sadly, he often took himself too seriously. Indeed, I think he did so much of the time, for he seemed to lack a sense of humor, at least where his utterances about ecology were concerned.

To give an example: holding forth on the contribution of F.E. Clements to the field of ecology, especially as regards climax concepts, Dauby suddenly became reiterative, stating that the trouble with American ecology was that everything important in the field of synecology was discovered by Clements, so much so that one might characterize its history as "Before Clements, B.C., B.C., B.C. . .." he finally added, "before Christ" with a full grin, Cheshire-like, something unusual for him; clearly, he much appreciated his effective presentation and original commentary. The class (about 60, mostly graduate students from several disciplines, for Dauby's classes were very popular) laughed appreciatively, including myself, but I raised my hand almost immediately after his riveting delivery and interjected rather loudly, and with much glee, and some laughter, "I now take it we're entering A.D., after Daubenmire!"

Instead of appreciating my joshing spontaneity, he became suddenly furious. Red faced and with grin-turned scowl, he ordered me out of the classroom "Out," he said, "Get out." The class was bewildered, for they had all chortled loudly at my retort, so was I, for I never meant to be disrespectful, merely entertaining, attempting to add to the pedagogic verbalization he'd seized upon.

I did leave the class as instructed, very embarrassed of course, although pleased that my peers had perceived my spontaneous remarks as somehow appropriate. Afterwards I tried to apologize to Dauby, but he would have none of it, although he did relent and permitted me back in his class the following week.

My interpersonal relationships with Dauby were largely developed because of my interest and background in plant systematics. I believe he sought out my conversation, both during field courses in connection with his formal classes in synecology, where sack lunches were the rule, and following this or that class lecture in which allusions were made to the views of systematists generally. I believe he mostly wanted feedback on his many attempts to make plant community classification "analogous" to organismal classification. "But they are very different," I would assert, "Community ecologists do not have evolutionary theory as a direct underpinning by which to arrange and classify." "Ah," he would respond, "communities evolve, they are made up of plants and animals, all of which coevolve," etc. And he would usually wrap up the conversation pretty quickly with terse sentences that made his points; (Dauby would have made an excellent trial lawyer speaking before an educated jury). Deep down, I think he knew these analogies were basically misleading, dishonest even, for he not only was well aware of Gleason's (a systematist!) individualistic concepts on community structure but, at the time also coexisted with Prof. R.H. Whitaker, his nemesis at Washington State University during my formative years there.

Like most academic professionals, Dauby had considerable concern about his standing in the field of plant ecology, especially as perceived by his peers. I remember well his deep sense of betrayal by the ecological community, if not the man, when the article by Frank Egler, "A commentary on American plant ecology, based on the textbooks of 1947-1949," first appeared in the October, 1951 issue of *Ecology* (32: 673-695). Egler, a very perceptive, erudite, human, to judge from his well-turned article, compared the ecological texts of F.E. Clements, *Dynamics of Vegetation*, 1949; H.J. Oosting, *The Study of Plant Communities*, 1948; and Daubenmire, *Plants and Environment* (*A Text Book of Plant Ecology*). Not only did Egler compare these texts (as indicators of the state of American plant ecology and its development over half a century), he also commented rather freely on the psyches of the authors concerned, especially as related to their academic beginnings. In preparing the present "obit", I re-read Egler's article (after a 44 year hiatus!) and it stills reads as I remember it from my first reading in 1951: a very personal evaluation by a highly skilled communicator with a broad grasp of his field. And he was clearly aware of the controversial nature of his commentaries, noting in his "Postlude," near the end of his article:

> I have been accused in this manuscript, both of being holier-than-thou, and of being satanic. With either accusation, I plead that to be both forceful and modest at the same time is a difficult task. If I appear to claim that I can see farther and from greater heights than some others, it is only - to use Newton's oft-quoted analogy - that those few cubits of stature have been attained by

climbing on the backs of giants. The giants are there for others to climb, even though the shoulders may bear us ungraciously.

In the fall of 1951 I was enrolled in Daubenmire's course in autecology, for which his text was mandatory, as noted above. I had not given much thought as to how the text might have been written, but after reading Egler's comments, I developed a greater interest in Dauby's style.

Dauby was undoubtedly flattered that Egler possibly ranked him as among the "giants" of American ecology, but Egler was surely correct that the "shoulders [of such workers] may bear us ungraciously." At least that seemed true of Dauby, who brought up Egler's article time and again during the late fall of 1951, complaining that the editors of *Ecology* should ever have published such a commentary. But what most galled him was Egler's paragraph on Dauby's "style of writing," which, in contrast with Clement's style, was said to have

> . . . succeeded to a high degree in developing a terseness, a paucity of words, a fact-crammed grammatical structure that is the goal of many a scientific writer. It is as functional, as devoid of decorative flourishes and artistic ornamentation as the layercake skyscrapers built lately in New York. As was said by the romanticist against the classicist, his writing had become correct and soulless, learned and uninspiring, scientific and godless, virtuous and cold. One can almost imagine that this author, beginning with terse abbreviated lecture notes, kept building through the years in card-catalogue style, inserting abstracts and summaries in their appropriate places as the new literature appeared. For these reasons, the book will long serve as a well-organized reference work for the American literature on the effects of environmental factors on plants.

And that was the way he lectured too, in both undergraduate and graduate courses, except in his autecology course, in which he never lectured, as noted in the above (the text seemingly written from abbreviated sentences on stacks of cards) with practically no sidebar diversions, even when controversy arose from among the students. And, too, that was the way he must have composed his text on Plant Geography (Academic Press, 1978). I attended his first class towards this new textbook venture in the spring of 1953, just before my doctoral defense scheduled for that same semester. My final personal insights into the man's ouvre and psyche involves that class.

I truly looked forward to Dauby's course. Having had a firm background in both plant geography and geology as a result of my master's work at Southern Methodist University in 1949-50, to say nothing of my courses in geomorphology and genetics at W.S.U., I felt primed and excited. Dauby even questioned my "need" to take his course, especially since I had made top grades in nearly all of my courses, and he was well aware of my conversational ability in systematics generally. "Concentrate on your doctoral thesis" he advised, knowing that I was scheduled to finish that same semester. But I told him my thesis was essentially written and that I would truly enjoy the class, *etc.* As a member of my doctoral committee, he relented.

Everything went fine in the course on Plant Geography. Dauby each day perfectly poised and academic, covering the topic from 5 × 8 cards with information not especially new or novel, throwing in this or that study called to the fore since Cain's fine text on the subject, *Foundations of Plant Geography*, which first appeared in 1944. Nothing new really, until suddenly one day he digressed. Lecturing upon the origin of American deserts and their likely age, he bedazzled me (but perhaps not the class) with his observation that the deserts had developed very recently in North America, and that their floras were probably derived out of mostly recently extinct if not extant elements of the more temperate *Artemisia* shrublands and grasslands of the western Rocky Mountains, if not from conifer forests. The kingpin in this hypothesis, he reckoned, was the fossil *Opuntia* described by Chaney from the Green River shales of Utah, "the earliest and perhaps only fossil cactus from the New World" he noted. "We have to be objective and acknowledge the evidence," he continued, drawing the words out tersely, and afterwards donning that smug Gioconda smile he was so adept at when playing his verbal trump cards.

I disagreed, of course, noting in class, lawyer-like perhaps, that all of the floristic evidence argued against his views: the Cactaceae is not well developed in temperate North America, anyway, if an *Opuntia* had happened to become fossilized in Eocene time, then it merely proved the cacti had been around for eons, and that the center of diversity of cacti in North America lay to the south in Arizona, New Mexico, Texas, mostly subtropical regions, much as suggested by Chaney in his paper, and what about *Fouquieria*, *Idria* (both belonging to the Fouquieriaceae, a family of only two genera confined to the hot deserts of North America without clear familial relationship elsewhere) and many other genera too numerous to mention, to say nothing of the genus *Larrea* which dominates the deserts of two continents, etc. On like that I held forth, and Dauby fumed, even entered this fray with a dead look of castigation. "I stand on the fossil data" he said, but noting at the same time that the state of Florida has as many cacti nearly as Arizona or New Mexico, and "certainly Florida is not a desert." "But the Florida cacti mostly belong to the genus *Opuntia*," I said, "many of these, if not most, of recent introduction or else the results of Small's taxonomic splitting of this or that variable entity. Anyway," I retorted, "The cacti of Florida, so far as evidence bearing on the age and origin of the family Cactaceae, is meaningless." And I forget, now, how our 15 minute debate went, but it ended with a stony silence on Dauby's part, and "I wish you weren't here" - look and an early closure of the lecture for that day.

After that venture into Dauby's card session, upon the advice of my graduate student peers, I kept strictly quiet, dutifully recording his lectures in my own shorthand in preparation for our final exam, which was soon upon us.

The exam was well-structured, very fair, and straightforward, as were all of the exams in the four courses I took from him. But for me, on this particular exam, there was a problem. Dauby asked the question (assigning it 10 points): Give the age and origin of the family Cactaceae (not worded so as to be answered, <u>according to Daubenmire</u>!). Nevertheless, I placed in the appropriate space provided the answer according to Daubenmire, recounting his views very nicely I thought. But at the bottom of my answer I wrote "This is the answer which you might wish, Dr. Daubenmire, but for the <u>correct</u> answer, see the backside of this sheet." There I defended <u>my</u> point of view (and those of many others) regarding this issue.

When the final exam was graded and the semester grades posted, I was surprised to see that I had received a 90 on my final exam (the entire cactus question graded as incorrect) and a B in the course. I inquired of him why he did not accept my answer to the cactus question concerned. His response was "Well, Turner, you got the answer, but you didn't believe it, or else why did you give an additional answer on the back side of the sheet; in short, you only get to give one answer, not two, that's why you missed the whole question!"

"OK," I said, "But what about the B in the course. I had A's in my earlier exams, and a low A (90) on the final, why a B? Other students with much lower averages received A's [I'd made comparisons among my peers]." "Well," he responded, "let's put it this way, you got a B for Bad Behavior," his eyes full on me dead as a desert duck, no water anywhere.

"Fine," I responded, laughing, "now that I know the standards I won't complain, considering the criteria I'm sure I got it fairly." That was one of the few B's I received in my university education and one that I am proudest of.

But the cactus question did not end there. Daubenmire attended my final defense (of a systematic thesis, a cytotaxonomic study of the genus *Hymenopappus*). After most of my committee members had finished asking this or that question, Dauby, who had said nothing to this point, suddenly said, "Turner, when and where do you think the Cactaceae arose?" I was taken aback, but rising to the occasion (I hoped then), I said strongly and affirmatively, without a glimmer of a smile, "Well, Dr. Daubenmire, do you want my answer, or yours?"

Dauby looked very distressed at my response, folded his papers, got up from the large table which was surrounded by about ten professors, and left the room. He did not approve my performance, but (so I was told) the upper administration, appraised his evaluation negatively and I passed my defense without undue rancor.

As a postscript to the cactus story recounted above, I can't help but add that the fossil *Opuntia* described by Chaney from the fossil beds was, some 18 years later, found to be to a fossilized rhizome and associated root system of a monocot, possibly a sedge (Becker 1962). Upon reading this "inspiring" revelation I sent copy of the article to Dauby, with a little memo, merely stating, "Remember this?" He never responded. Nor did he include an account of his views on the origin of the Cactaceae in his text on Plant Geography. Indeed, published some 25 years after that first class on the subject, Dauby's outlook re American deserts changed considerably, even introducing in his text some of the very same views which I propounded in his first course on the subject.

I hope the above account is not viewed by the reader as a "get-even" article. It is not intended as such (to my knowledge). Rather, I hope in this telling to capture an aspect of the man not generally known. Like most of us he had a mixture of traits some admirable, some not. But, surely some of these affected his research and teaching. In fact, I consider him with his often adamant views and determination to be the foremost ecologist in America (during his heyday) the essential ingredients of most successful scientists. Even at the time I admired his competitive personality, although disagreeing, upon occasion, with his behavior. Certainly he was one of the most organized, clearly focused graduate level teachers to position information in my neural lodgings.

Dauby was the academic father of numerous doctoral students in ecology, many of these friends of mine. For the most part he kept them at a distance; some he favored with warm, but detached, smiles and relatively brief office conferences; others he simply ignored, doubting their competence, begrudgingly entering into their research projects and practically never into their personal problems. Most of his students appeared to stand in awe of the man, even forming cabals among themselves and their leader, constituting a solid phalanx whenever Dauby's views were attacked by W.H. Whitaker or yet others. But that is another telling.

LITERATURE CITED

Becker, H. 1962. Reassignment of *Opuntia* to *Cyperacites*. Bull. Torrey Bot. Club 89:319-330.

Turner, B.L. 1972. Lowell David Flyr, 1937-1971. Sida 5:54-58.

____. 1975. Marion Ownbey 1910-1971, an appreciation. Pl. Sci. Bull. 5:56-58.

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